ROYAL ASTRONOMICAL SOCIETY.

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G. B. AIRY, Esq., President, in the Chair.

Robert Brewin, Jun., Esq., Birstall Hall, Leicestershire;
Benj. Dennison Naylor, Esq., Altrincham, Cheshire;
were balloted for and duly elected Fellows of the Society.

Extract of a Letter from Professor Hansen to the Astronomer Royal, dated 1853, *Nov.* 4.

"The work on the Lunar Tables has been greatly advanced during this last year. In the course of a fortnight we shall arrive at the definitive corrections of the elliptic elements for the present epoch, as well as the coefficients of the disturbances which depend on the sun's parallax and on the ellipticity of the earth; and I count upon an excellent agreement between the Observations and the Tables. For greater security these corrections are separately based on the Greenwich and the Dorpat observations; and, as far as I can foresee at present, a very good agreement will in this respect also be presented. From the discussion of these observations, however, it results that something yet remains depending on the difference of longitude between Greenwich and Dorpat. I have to-day written to Struve on this subject. The work which yet remains is the settling of the Epoch of the Moon's motion for remote times for the definitive determination of the mean motion. I hope, in the space of a year, to be quite ready with the Lunar Tables.

"The Solar Tables are at last printed."

Observations of Comet II., 1853, *made at the Royal Observatory, Cape of Good Hope.* By Thomas Maclear, Esq.

When the comet was detected shortly after nightfall on the 1st of May, the tail appeared to be about $3\frac{1}{2}$ or 4 degrees in length, and singularly curved or convex towards the south, near the head, conveying to the imagination the impression of a parabola, of which

the curve on the south side of the axis only was visible. The outline of the parabolic shaped head was sharply defined, and it seemed to be immersed in a very delicate coma or halo, which subsequently increased in dimensions.

In the direction of the axes there was no cometary matter, nor within the curve anywhere up to the nucleus; and the edge of the tail adjacent to the axis appeared to be sharper than the opposite edge.

In brilliancy, the nucleus might be safely taken equal to that of a star of the fifth magnitude; but, under high powers, the pungency of the light vanished, and the image spread fan-shaped in the direction of the head. It remained pretty bright until the 9th of May.

With respect to the observations, they were made by myself with the $8\frac{1}{2}$-feet equatoreal and spider-line micrometer, armed with the power 123; and, in general, the telescope was moved by the "driving clock," when the star and comet were simultaneously visible in the field.

It may be proper here to state, that a single micrometer measure for difference in right ascension seems to be at least six times more accurate than the difference by time transits over a single wire, where the objects are pretty well defined; and perhaps twenty times more accurate when one of them is a nebulous blotch, whose centre has to be estimated. There is a strong instance of this on the 7th of June.

For several days before its final disappearance, the comet resembled an oval-shaped nebula, slightly condensed in the middle. The last observation was on the 11th of June; the following three nights were clouded; afterwards it could not be detected.

The first and second columns give the date and Cape mean time of observation. The third column contains the observed differences in right ascension, corrected for refraction, between the comet and stars compared with it when derived from transits; or, when observed in terms of the micrometer-screw, the arc value reduced to time and multiplied by the sine of the N.P.D. of the middle point between the comet and the star. The fourth column gives the number of partial measures. The fifth gives the arc values, corrected for refraction, of the observed differences in N.P.D.; and the sixth gives the number of partial measures.

The signs + and —, prefixed to the differences in columns three and five, imply that the quantities are to be added to or subtracted from the R.A. and N.P.D. of the stars to obtain the R.A. and N.P.D. of the comet.

The seventh column gives the conventional numbers of the stars for reference. Their approximate places derived from the equatoreal circles, will be found on the last page sufficiently near for identification. Their correct positions will be observed at the proper season with the meridian instruments.

Royal Observatory, Cape of Good Hope,
Aug. 13, 1853.

1853.	Cape Mean Time.	Difference in R.A.	No. of Obs. in R.A.	Difference in N.P.D.	No. of Obs. in N.P.D.	No. of Star.	
	h m s	m s		′ ″			
May 1	7 46 28·10	.....	...	+ 2 50·24	2	1	
	7 56 37·29	−1 46·532	2	.....	...	1	*t*
	8 6 17·36	.....	...	+ 3 38·17	2	1	
	8 26 37·32	.....	...	+ 4 25·86	5	1	
	8 37 17·28	0 34·479	1	.....	...	1	*t*
3	7 19 49·87	.....	...	+ 5 12·82	3	2	
	7 42 23·62	1 12·658	4	.....	...	2	*t*
	7 42 23·62	1 34·739	4	.....	...	3	*t*
	7 42 23·62	1 53·530	4	.....	...	4	*t*
	7 56 28·52	.....	...	+ 5 25·34	...	2	
	8 10 29·46	0 50·447	3	.....	...	2	*t*
	9 11 36·35	−0 1·623	8	.....	...	2	*t*
	9 18 32·71	.....	...	+ 5 41·45	...	2	
4	6 35 11·59	.....	...	− 4 56·02	2	5	
	6 57 18·62	+0 34·348	9	.....	...	5	*t*
	6 57 18·62	−0 17·970	9	.....	...	6	*t*
	7 14 42·60	.....	...	− 3 19·55	2	6	
	7 22 54·73	−0 3·370	9	.....	...	6	*t*
	7 35 59·60	.....	...	− 3 20·31	2	6	
	8 8 11·72	+0 25·340	5	.....	...	6	*t*
	8 30 21·54	.....	...	− 3 19·85	3	6	
	8 44 49·83	+0 44·486	3	.....	...	6	*t*
	8 10 53·45	−1 22·002	3	.....	...	7	*t*
	8 30 21·54	.....	...	− 1 4·42	3	7	
	8 50 28·63	−0 58·841	6	.....	...	7	*t*
	9 13 5·16	.....	...	− 1 7·67	2	7	
5	7 0 27·44	.....	...	+11 28·42	1	8	
	7 37 17·49	−7 29·912	1	.....	...	8	*t*
7	7 3 43·40	+1 14·379	4	.....	...	9	*t*
	7 19 37·18	.....	...	−10 13·21	2	9	
	7 36 1·49	+1 22·408	3	.....	...	9	*t*
	8 3 37·29	.....	...	−10 23·67	3	9	
	9 3 57·54	.....	...	−10 31·27	2	9	
	9 11 16·84	+1 47·781	1	.....	...	9	*t*
	7 3 43·40	−0 46·079	4	.....	...	10	*t*

Clouded on the 2d.

May 5. Owing to clouds these observations only could be obtained.

7. Angle of position of comet's tail, 118° 40′.
No. 10 is in the comet's tail.

1853.	Cape Mean Time.	Difference in R.A.	No. of Obs. in R.A.	Difference in N.P.D.	No. of Obs. in N.P.D.	No. of Star.	
	h m s	m s		′ ″			
May 7	7 25 34·21	.....	...	− 7 36·70	2	10	
	7 36 1·49	−0 37·838	3	.....	...	10	*t*
	8 35 1·80	.....	...	− 7 52·92	2	10	
	9 11 16·84	−0 12·661	1	.....	...	10	*t*
8	6 23 36·26	−0 34·350	4	.....	...	11	*t*
	6 44 36·46	.....	...	− 1 28·88	2	11	
	6 23 36·26	−1 1·502	4	.....	...	12	*t*
	6 50 2·67	.....	...	+ 3 59·82	2	12	
	6 23 36·26	−2 20·278	4	.....	...	13	*t*
	7 0 22·20	.....	...	+10 54·81	3	13	
9	7 2 3·47	+2 31·153	2	.....	...	13	*t*
	7 30 53·78	+2 36·305	1	.....	...	13	*t*
	7 43 20·78	.....	...	+ 6 47·17	3	13	
	8 2 31·62	+2 42·029	5	.....	...	13	*t*
	8 19 33·00	.....	...	+ 6 39·63	2	13	
10	7 11 44·27	.....	...	− 3 6·40	2	14	
	7 30 39·76	−0 27·602	1	.....	...	14	*t*
11	6 36 25·27	+0 44·368	3	.....	...	15	*t*
	7 0 34·48	.....	...	+ 5 37·06	4	15	
	7 21 27·38	+0 50·129	4	.....	...	15	*t*
	8 12 59·09	.....	...	+ 5 20·65	4	15	
	8 32 5·06	+0 58·440	4	.....	...	15	*t*
	6 36 25·27	−1 37·933	3	.....	...	16	*t*
	7 0 34·48	.....	...	+ 1 2·07	4	16	
	7 21 27·38	−1 32·201	4	.....	...	16	*t*
	8 12 59·09	.....	...	+ 0 45·13	4	16	
	8 32 5·06	−1 23·953	4	.....	...	16	*t*
12	6 41 44·35	.....	...	+ 1 20·44	6	17	
	6 54 57·74	−0 1·429	8	.....	...	17	*d*
	7 2 55·05	−0 0·686	9	.....	...	17	*d*
	7 15 7·60	.....	...	+ 1 14·82	9	17	
	7 36 9·97	+0 2·506	14	.....	...	17	*d*
	8 10 38·13	.....	...	+ 1 5·85	5	17	
	8 19 36·27	+0 6·752	5	.....	...	17	*d*

May 9. Angle of position of comet's tail, 121° 30′. Nucleus remarkably bright.
10. Clouded after these observations.
11. Angle of position of comet's tail, 115° 40′. Nucleus less bright; halo round the head more diffused.
12. Angle of position of comet's tail, 117° 30′.

1853.	Cape Mean Time.	Difference in R.A.	No. of Obs. in R.A.	Difference in N.P.D.	No. of Obs. in N.P.D.	No. of Star.	
	h m s	m s		′ ″			
May 14	7 27 56·82	.....	...	− 5 52·53	3	17	
	8 4 32·88	+4 28·769	5	.....	...	17	*t*
	8 42 50·40	.....	...	− 6 6·18	5	17	
15	6 32 58·31	.....	...	+ 3 34·95	5	18	
	6 49 34·79	−0 37·701	5	.....	...	18	*t*
	7 7 42·58	−0 34·606	5	.....	...	18	*d*
	7 24 52·76	.....	...	+ 3 27·48	5	18	
	7 40 2·46	−0 32·717	5	.....	...	18	*d*
	7 49 22·93	.....	...	+ 3 22·71	5	18	
	8 0 34·68	−0 31·393	5	.....	...	18	*d*
	8 11 40·64	.....	...	+ 3 20·37	5	18	
16	6 20 43·76	.....	...	+ 7 42·41	5	19	
	6 32 35·81	−0 26·035	5	.....	...	19	*d*
	6 40 49·26	.....	...	+ 7 38·85	5	19	
	6 53 34·76	−0 24·743	5	.....	...	19	*d*
	7 3 28·35	.....	..	+ 7 37·75	5	19	
	7 14 24·55	−0 23·516	5	.....	...	19	*d*
	7 21 15·63	.....	...	+ 7 33·79	5	19	
	7 37 17·40	−0 21·992	5	.....	...	19	*d*
	7 49 10·05	.....	...	+ 7 28·26	5	19	
	7 57 44·84	−0 20·916	5	.....	...	19	*d*
	8 21 20·18	.....	...	+ 7 23·12	5	19	
17	6 15 6·14	−0 16·685	5	.....	...	20	*d*
	6 16 0·00	−0 21·075	5	.....	...	21	*d*
	6 58 51·77	.....	...	− 7 56·53	5	20	
	7 5 4·55	.....	...	+ 3 26·32	5	21	
	7 16 43·34	−0 13·617	5	.....	...	20	*d*
	7 18 12·00	−0 18·014	5	.....	...	21	*d*
	7 28 54·65	.....	...	− 8 0·03	5	20	
	7 34 51·87	.....	...	+ 3 26·51	5	21	
	7 47 36·38	−0 12·086	5	.....	...	20	*d*
	7 48 38·21	−0 16·387	5	.....	...	21	*d*
	8 4 19·63	.....	...	− 8 0·31	...	20	
	8 9 58·11	.....	...	+ 3 17·34	5	21	
20	6 42 20·46	+0 12·428	5	.....	...	22	*d*

May 14. Angle of position of comet's tail, 118°. Clouds.
15. Angle of position of comet's tail, 117° 35′.
17. Angle of position of comet's tail, 118° 45′.
20. The comet is faint, partly owing to moonlight. The tail barely distinguishable.

1853.	Cape Mean Time.	Difference in R.A.	No. of Obs. in R.A.	Difference in N.P.D.	No. of Obs. in N.P.D.	No. of Star.	
	h m s	m s		′ ″			
May 20	6 49 23·50	.....	...	− 0 48·61	5	22	
	6 55 1·77	+0 11·957	5	.....	...	22	*d*
	7 2 55·48	.....	...	− 0 51·63	5	22	
	7 8 59·68	+0 11·303	5	.....	...	22	*d*
	7 15 43·57	.....	...	− 0 50·25	5	22	
	7 21 17·66	+0 10·887	5	.....	...	22	*d*
21	6 41 26·83	.....	...	+ 6 3·15	5	23	
	6 49 47·26	−0 14·373	5	.....	...	23	*d*
	6 55 44·28	.....	...	+ 6 3·05	5	23	
	7 2 29·57	−0 14·653	5	.....	...	23	*d*
	7 9 28·82	.....	...	+ 6 1·23	5	23	
23	6 35 26·93	.....	...	+ 7 51·42	5	24	
	6 48 36·57	−0 2·714	5	.....	...	24	*d*
	7 0 21·44	−0 2·586	5	.....	...	24	*d*
	7 9 41·51	−0 2·157	5	.....	...	24	*d*
	7 18 44·82	.....	...	+ 7 41·29	5	24	
28	6 27 55·21	.....	...	+ 4 55·45	5	25	
	6 45 59·48	−0 30·305	5	.....	...	25	*d*
	6 56 48·34	−0 30·334	5	.....	...	25	*d*
	7 6 1·61	.....	...	+ 4 51·97	5	25	
29	6 13 20·56	−0 6·578	5	.....	...	25	*d*
	6 20 29·69	.....	...	+ 2 32·30	5	25	
	6 29 17·02	−0 6·249	5	.....	...	25	*d*
	6 35 10·81	.....	...	+ 2 30·40	5	25	
	6 40 52·64	−0 5·963	5	.....	...	...	*d*
31	6 24 32·60	+0 37·004	5	.....	...	25	*d*
	6 31 0·12	.....	...	− 2 5·16	5	25	
	6 37 46·37	+0 36·985	5	.....	...	25	*d*
	6 46 57·97	.....	...	− 2 9·65	5	25	
	6 54 6·78	+0 36·935	5	.....	...	25	*d*
June 1	6 27 12·98	.....	...	+10 2·01	5	26	
	6 34 57·26	−0 47·934	5	.....	...	26	*d*
	6 41 52·68	.....	...	+ 9 58·94	5	26	
	6 50 17·87	−0 47·377	5	.....	...	26	*d*
	7 14 34·44	−0 48·083	3	.....	...	26	*d*

May 23. Thin clouds. The comet is very indistinct.
29. The comet is very faint.
June 1. The measurements diminish in precision, owing to the faintness of the comet.

1853.	Cape Mean Time.	Difference in R.A.	No. of Obs. in R.A.	Difference in N.P.D.	No. of Obs. in N.P.D.	No. of Star.	
	h m s	m s		′ ″			
June 1	7 14 34·44	+1 0·237	3	.....	...	25	*t*
	7 24 49·65	.....	...	− 4 23·62	5	25	
2	6 27 25·54	+1 18·965	3	.....	...	25	*t*
	6 45 16·64	.....	...	−6 26·94	4	25	
	7 1 22·02	+1 19·253	2	.....	...	25	*t*
	6 27 25·54	−1 24·261	3	.....	...	27	*t*
	6 45 16·64	.....	...	−2 53·07	4	27	
	7 1 22·02	−1 23·896	2	.....	...	27	*t*
3	6 36 53·39	.....	...	+5 54·40	5	26	
	6 45 8·39	−0 11·407	5	.....	...	26	*d*
	6 52 20·58	.....	...	+5 53·80	5	26	
4	7 7 22·67	.....	...	+3 53·75	1	26	
	7 13 12·69	−0 5·529	2	.....	...	26	*d*
	7 25 19·64	.....	...	+3 47·42	2	26	
5	6 34 48·27	.....	...	+2 4·69	3	26	
	6 45 21·34	+0 21·519	5	.....	...	26	*d*
	6 55 12·79	.....	...	+2 3·91	3	26	
7	6 35 58·61	.....	...	−1 22·35	3	26	
	6 47 3·93	+0 51·866	10	.....	...	26	*d*
	6 58 43·86	.....	...	−1 24·26	5	26	
	7 16 36·13	+0 54·550	10	.....	...	26	*t*
	7 31 40·34	.....	...	−1 26·34	2	26	
8	6 49 9·52	+1 8·199	5	.....	...	26	*t*
	7 7 54·37	.....	...	−2 59·49	10	26	
	7 26 44·26	+1 9·505	3	.....	...	26	*t*
9	6 23 37·43	.....	...	−4 22·13	5	26	
	6 38 11·42	+1 23·619	7	.....	...	...	*t*
	6 56 13·48	.....	...	−4 23·55	5	26	
10	6 23 48·76	.....	...	−5 47·12	5	26	
	6 39 39·45	+1 38·681	5	.....	...	26	*t*
	6 54 45·86	.....	...	−5 48·28	5	26	
11	6 34 14·35	.....	...	+4 51·74	5	28	
	6 45 47·37	−0 22·350	5	.....	...	28	*d*
	6 55 0·64	.....	...	+4 50·27	5	28	

June 3. The comet is very faint.
4. Generally clouded. These observations only could be obtained.
8. Hazy atmosphere. The measures very difficult.
10. The moonlight is now diminishing the visibility of the comet.

Approximate Positions of the Stars compared with the Comet.

Reference Number.	R.A.	N.P.D.	Mag.	
	h m s	° ′ ″		
1	5 0 2	103 19 23	7	Preceded by a small star
2	5 57 29	104 4 54	7	H. C. No. 11551
3	5 57 51	104 1 37	8½	
4	5 58 10	104 6 51	8½	
5	6 11 52	104 18 18	10	
6	6 12 44	104 16 42	9	
7	6 14 32	104 14 30	8	
8	6 32 32	104 1 0	5	H. C. No. 12825
9	6 40 7	104 16 15	6	B. A. C. 2221
10	6 42 8	104 13 38	9?	
11	6 47 36	104 3 47	9½	
12	6 48 2	103 58 21	8	H. C. No. 13392
13	6 49 21	103 51 30	5½	B.A.C. No.2273 : μ Canis Majoris
14	6 56 20	103 57 20	9	
15	6 58 15	103 45 0	8½	
16	7 0 37	103 49 30	8	H. C. No. 13848
17	7 1 49	103 45 28	9	
18	7 8 37	103 32 48	8	
19	7 9 59	103 25 24	7?	
20	7 11 10	103 37 49	7½	H. C. No. 14228
21	7 11 14	103 30 18	8	
22	7 14 22	103 21 24	10	
23	7 14 46	103 11 35	10	
24	7 16 34	103 4 9	10	
25	7 19 45	102 53 48	7½	
26	7 21 35	102 39 30	9	Preceded by a faint star, nearly on the same parallel.
27	7 22 28	102 50 20	10	
28	7 23 50	102 28 50	10½	

Extract of a Letter from A. G. Constable, Esq. to the Astronomer Royal, dated Lima, June 1853.

"I extract from my journal, premising that the bearings, &c., I received from the captain of the ship (in which I was only a passenger from Melbourne to Callao), are correct.

"May 1, 1853. This evening, at 7 minutes to 7 p.m., observed a comet at an altitude of 20° 47′ 30″, bearing W. by S. ½ S., dis-

tant from the lowest star in *Orion's* belt (*Bellatrix*), 16° 29′ 30″, and bearing from that star about S.S.W.

"At noon of that day our latitude was 34° 39′ 8″, and longitude 167° 54′ 4″, and since then we have been steering towards the N.W. end of New Zealand with a six-knot breeze.

"The comet appeared to be a star of the first magnitude, rather dim, and of a reddish hue, with a bright tail of 4° in length.

"P.S. Although anxiously watched for, the comet did not again appear to us."

The following communication (referred to in the Supplement to the June *Notice*) has been received from Capt. Shadwell, containing an account of physical observations of Comet II., 1853, made at the Mauritius by M. Bosquet, the Government Meteorological Observer at Port Louis.

"The Observatory of which M. Bosquet has charge is one of a purely meteorological character, and is unfurnished with any astronomical instruments, so that on the appearance of the comet that gentleman was only able to record the general physical appearance exhibited by that body.

"April 28. Soon after sunset there was a remarkable appearance of the zodiacal light, rising in a conical form to an altitude of about 50°, and extending into the constellation of *Orion*. This phenomenon lasted $6^h\ 45^m$ p.m.

"On the 29th and 30th, the evenings were more or less obscured by clouds.

"On May 1st, M. Bosquet distinctly observed the comet, and thus describes its appearance. The nucleus appeared with the brilliancy of a fourth-sized star, and the tail, nearly 10° long, was projected from the sun towards the constellation of *Orion*. The nucleus was surrounded by a vapoury appearance.

"On May 2d the tail of the comet seemed to have considerably diminished in length and brilliancy. The nucleus appeared smaller, and no bright point could be observed in the middle with the naked eye, as on the night preceding. The nucleus was nearly in a straight line between *ε Orionis* and *α Leporis*.

"May 3. During the evening the weather was very clear, and the whole body and tail of the comet could be seen of a faint and pale colour. With the naked eye the nucleus seemed of a very pale light, and no bright points could be discovered within it; but viewed through a telescope, with a power of 60, a node or body was distinctly visible in the nucleus. The tail projecting towards *β Canis Majoris*, extended over it as well as over some other stars of the 4th and 5th magnitude, which could be distinctly seen through it.

"May 4th. The comet this evening had again diminished in size and brightness. To the naked eye it seemed as a faint light like a cluster of nebulæ, and had a very short tail. With a telescope the tail could be distinctly observed; its direction was slightly changed,

being a little more inclined towards the southward, and diverging more from the equator.

"May 5th. Comet gradually vanishing; stormy to-night; a faint whitish light. No observations taken subsequently to the 5th. The comet remained faintly visible till the 8th.

"*Note.* The estimated measurements of the length of the tail, as recorded both by Mr. Wright and M. Bosquet, were eye estimations, and not in any case obtained instrumentally.

"Charles F. A. Shadwell, Capt. R.N.

"P.S. Finding, on comparing notes with Mr. Maclear at the Cape of Good Hope, that the comet had not been observed at the Cape before the 1st of May, and, therefore, that the observation of April 30th on board the Lady Jocelyn would be valuable, I have recomputed the comet's place, correcting it for refraction, and find as follows:—

App. Gr. Mean Time, $4^h\ 17^m$, April 30th,
R.A. $3^h\ 57^m\ 36^s{\cdot}3$ Decl. $11°\ 26'{\cdot}4$

using Dr. Inman's Refraction Tables, which are based on Dr. Young's formulæ."* C. S.

Extract of a Letter from Lieut. John Parish, commanding H.M.S. Sharpshooter at Rio de Janeiro, May 14, 1853.

"We had a very good sight of a comet whilst out on our last cruise, on the night of the 30th of April, bearing W. $\frac{1}{2}$ S. from our anchorage in Anuaçoa Bay, about 20 miles north of Rio Janeiro. Altitude 9° 2′, distance from *Sirius*, 35° 32′, from *Bellatrix*, 23° 51′, length of tail from 8° to 10°. Visible also on the night of the 1st of May, in nearly the same position. On our return I found it had been seen at Rio de Janeiro on the night of the 30th April.

"Has it been seen in Europe? If not this account of it may be interesting to astronomers.

"Had the weather been fine, I could have given you more particulars. It was first seen about $7^h\ 20^m$ p.m., and set at about 8 p.m."

Extract of a Letter from Capt. King, of Paramatta, N.S.W.

"April 30. A comet suddenly made its appearance after sunset. Angles from it to *Sirius*, 44° 19′, *Rigel*, 22° 35′, *Bellatrix*, 30° 20′.

"May 1. Clouded.

"May 2. Passing between λ and ν *Leporis*, about 18° 35′ from *Sirius*, towards which it was travelling. R.A. about $5^h\ 14^m$, decl. 13° south. Comparing this position with its place on the 30th

* Capt. Shadwell had not at this time learned that the comet had been previously discovered in Europe.

April, it had changed in R.A. 25°, and in declination 2° to the south, which will give for its daily motion in R.A. 75′·0, and in declination 60′. There was no star sufficiently near it to ascertain its position with a ring micrometer.

" May 3. Sextant angle between the comet and the following stars. *Sirius*, 12° 5′, *β Canis Majoris*, 7° 30′ (?), *Canopus*, 38° 58′, *Procyon*, 32° 16′, *Betelgrave*, 21° 41′, and with a ring micrometer at $7^h\ 58^m\ 28^s$ mean time (measured from *η Leporis*), it was, in R.A. $5^h\ 58^m\ 42^s$, decl. 14° 8′ 50″ south.

" May 4. Very indistinctly seen with a night-glass, and not discernible through a telescope, is about R.A. $6^h\ 12^m$, decl. 15° 45′ south.

" When first seen on the 30th, it was a very beautiful object, the nucleus and coma of rather large dimensions, but much diffused in the hazy medium, for it was low in the western horizon. The coma was about 4° in length, and the nucleus apparently of one-fourth the dimensions of the full moon. On the 2d May its form was more defined, the coma more linear, and the nucleus had a stellar body of apparently half the size of *Jupiter*, and much resembling that planet as if seen through a mist. As the comet descended, this appearance vanished, and the nucleus resumed the form it had on the first night (30th). On the 3d the stellar nucleus was not visible. Otherwise, except in its reduced size, it was the same as seen on the 2d. On the 4th it was much more indistinct, and the succeeding nights were also too cloudy to see it.

" It is much to be regretted that this, now becoming very important part of the world, is not considered to be worthy of the re-establishment of the Observatory, particularly as there are now several good instruments rusting in cases in the Government stores.

" A time-ball establishment is talked of, and during the last Session of Council a sum of money was voted for its erection; but as yet nothing has been done towards its completion. It is purposed to adapt the observing apartment for two meridional instruments, probably a transit and a mural circle. Room is left for the addition of a dome-room, for an equatoreal or altazimuth; but nothing will be done unless there be some impulse from without."

Note on Comet III. 1853.

(*Extract of a Letter from Dr. Foster, of Brussels, dated October* 4, 1853.)

" On Sunday, the 28th of August, the atmosphere being clear of vapour, I got a very good observation, and was enabled to apply some high power to it, owing to the extreme transparency of the air. In every telescope it had the same *clear and well-defined globe appearance*, of a bright golden colour, which was very distinct from the coma."

Daylight Observations of Comet III. 1853.
By John Hartnup, Esq.

The rapid increase in the brightness of the comet towards the latter end of August led me to think it possible that it might be seen with the sun above the horizon, and fortunately the 3d of September turned out a favourable day for such observations. Cirrocumuli clouds were prevalent, but between those clouds there were patches of clear blue sky. At about a quarter before 1 p.m. I set the equatoreal to the position of the comet deduced from the ephemeris published in No. 869 of the *Astronomische Nachrichten;* and on applying my eye to the telescope, the comet was immediately detected near the centre of the field. Its appearance was planetary; the nucleus was round, well defined, of a whitish colour, and about 9″ diameter. I could not detect the slightest appearance of a tail. The power used was 92; but, in order to obtain the position of the comet more accurately, I put on the parallel wire micrometer (power 132), with which it was well seen: but before I could complete my arrangements for an observation, clouds came over, and I did not see it again till after 2 p.m. Between 2 and 4 p.m. I saw the comet again two or three times for very short intervals; but it was exceedingly faint, and clouds and haze prevented further observation.

The following observations were obtained in daylight sufficient for me to see the spider-lines in the micrometer without the aid of illumination :—

	Green. M.T.	R.A.	Log $\frac{p}{P}$	N.P.D.	Log $\frac{q}{P}$
1853.	h m s	h m s		° ′ ″	
Aug. 29	7 33 0·2	11 34 26·63	+8·617	74 12 15·27	−9·8930
	47 59·8	34 25·48	·613	13 37·33	·8987
30	7 29 17·6	11 32 38·28	+8·611	76 25 10·68	−9·8970

The observations are corrected for refraction.

The comparison star for all the observations was *β Leonis*, and the following is the assumed mean place for 1853·0, deduced from the Greenwich Observations, 1849–50 :—

R.A.	N.P.D.
h m s	° ′ ″
11 41 33·53	74 36 22·51

On the 29th, the tail, which was directed from the sun, consisted of two curved rays of light, which united at about three degrees from the nucleus.

Discovery of a New Planet at Mr. Bishop's Observatory.
By J. R. Hind, Esq.

"I have to announce my discovery of another new planet on the 8th of the present month, at $7^h\ 50^m$ mean time, not very far from the place where I found *Thalia* in December last. It shines

as a star of the ninth magnitude, and evidently belongs to the group between *Mars* and *Jupiter*.

"By micrometrical comparisons with No. 6361 of Lalande's Catalogue, the following positions were deduced:—

M.T. Regent's Park.	App. R.A.	App. Decl.
h m s	h m s	° ′ ″
Nov. 8 at 8 2 46·6	3 19 33·50	+16 4 32·3
— 8 34 1·2	32·17	28·4
— 11 21 58·4	24·93	11·4
9 — 9 2 31·1	3 18 30·93	+16 1 18·0

"The place of Lalande's star was taken from a Greenwich observation on November 9th, with which the Astronomer Royal kindly furnished me.

"*Mr. Bishop's Observatory, Nov.* 11."

This planet has received the name of *Euterpe*.

First Elements of Euterpe.

Computed from the Observations of Regent's Park, Nov. 8, Durham, Nov. 12 and Nov. 17, 1853, by Professor Chevallier and Mr. George Rümker.

Epoch, 1853, Nov. 20·0, Greenwich Mean Time.

M	326 24 19·4	
π	100 42 2·0	Mean Equinox,
λ	91 3 45·6	Jan. 0, 1854.
i	1 45 47·9	
ϕ	10 23 47·8	
Log a	0·385910	
Log μ	2·971142	

These elements represent the middle observation with an error (computed—observed) of —0″·1 in Longitude, and 0″·0 in Latitude.

Professor Chevallier has also caused to be printed an Ephemeris of the planet based on the foregoing elements, and extending from Nov. 20 to Dec. 31. Any person may obtain a copy of this Ephemeris by applying at the apartments of the Society, to which a supply of copies has been liberally forwarded by Professor Chevallier for distribution.

Notice respecting his Observatory at Red Hill.

By R. C. Carrington, Esq.

"It has been known to several of the members of the Society for some time past, that I have been preparing to resume my former occupation as an observer in a different part of the country, and with the intention of following a different line of observation to that which formed my duty at Durham. Many months were necessarily

taken up in finding a site suitable for a meridian observatory, in building, in providing instruments, and in getting into order again. As I have now been at work for three or four months, and can produce some results of observation, I think the time has come for giving some notice to the Society of the class of work I am engaged in.

"To be as brief as possible, I have planned my observatory entirely with reference to zone-observation on the meridian, and the formation of accurate maps of portions of the heavens. Any other subjects taken up will be temporary or subservient to the above. One such subject will probably be the positions of spots on the sun, if not found to interfere prejudicially with our time for reduction.

"In the execution of this plan I propose to establish one or two distinctions between any catalogue I may live to produce and those which we have already in existence. In the first place, I intend to take such measures as shall effectually guard against the omission of any star included within what is now called the tenth magnitude; secondly, that the zones of observation shall be incorporated into catalogue-zones of 9° extent in N.P.D., reduced to mean places of an epoch common to the zone, in which every star, to the tenth magnitude inclusive, within the limits, will have its reference number; and thirdly, that each star shall be sufficiently often observed, or otherwise checked, that no errors of whole minutes or graduations shall remain to render its present existence or future identity doubtful. With respect to the maps, I propose that while the scale shall be sufficiently large to enable configurations to be satisfactorily represented, the size of the maps themselves shall not exceed what is found convenient to hold in the hand while comparing with the telescope, and further, that the places of all nebulæ mistakable for comets shall be plainly indicated. To the above it will be my endeavour to add the searching out and approximate measurement of all multiple stars that have hitherto escaped notice, and the determination of the magnitude of each star by some more accurate standard than the unaided judgment.

"It is, I conceive, only by an united system of cataloguing and mapping that a faithful history of the heavens of our times can be transmitted, by which alone those changes can be detected, which will hereafter form new subjects of inquiry.

"It may be only my private opinion, but I consider that with the excellent catalogues of Bessel and Argelander, now so accessible, any future author of a catalogue pretending to the character of a survey would be doing less than the progress of astronomy now demands, who should not bind himself to some such rules as the above.

"The portion of the heavens to which my attention is now, and will be for some time, entirely confined, is the region within 9° of the north pole. I estimate that, to observe, reduce, and map the stars therein contained, will occupy me from two to three years, taking into account the climate.

"My present instruments are a transit-circle and a small equatoreal, constructed by Mr. Simms, who undertook both the optical and mechanical part, and who has not been unmindful of his well-earned reputation. The transit-circle has 5 inches aperture, $5\frac{1}{2}$ feet focal length, 5 feet axis, two circles of 40 inches diameter, the graduated one being divided in gold on a bevelled face, and read by four microscopes, arranged in a cone, as at Greenwich. There is a counterpoise apparatus by which the residual pressure is reduced to about 15 lbs. on each bearing. There are two fixed collimators of 30 inches focal length. The axis and circles are of gun-metal, the telescope cones of hammered brass. The piers contain about 10 tons of Caen stone, resting on a thick slab of York flag-stone, laid on a considerable mass of concrete,—arrangements which I superintended personally throughout. The building is so planned that the transit-room is bounded on both sides by occupied rooms, in which the fire-places are removed to the opposite sides: the party walls are of brick, 12 inches thick: the north and south walls are of stone, 14 inches thick, and *without windows*. The level error was $0''\cdot7$ on Aug. 1st, at starting, and $1''\cdot8$ on Nov. 1st, the change having been very gradual and pretty constantly watched: the meridian error has varied a very little more in amount. The only objection to the abolishment of windows that I have traced is, that it appears to favour the generation of spiders. The equatoreal has $4\frac{1}{2}$ inches aperture and $4^{ft.}\ 4^{in.}$ focal length. I usually employ a power of 28 and a field of a degree and a half: it is suited for rough zoning, sweeping, mapping, and screen-projection. It is German-mounted on a cast-iron stand of my own design, which I think possesses some advantages. The clock I now have was made by the late H. Appleton, of 30 Southampton Row, Russell Square: it has the dead-beat escapement; its rate is excellent, but the beat dull, and I have still some difficulty in this respect to get over before I can set at defiance the sounds of a busy neighbourhood.

"At the next meeting of the Society I hope to lay on the table printed copies of nine maps, containing the results (above alluded to) of a first preliminary exploration of the region I have named, and which was brought to a close within the present week. The scale is 1·5 inches to the degree, and 4268 stars have been laid down, the mapping having been purposely carried beyond the proposed limit for reasons which I will then explain.

"The approximate position of the observatory is

$51^{\circ}\ 14'\ 25''\cdot25$ North
$0^{m}\ 41^{s}\cdot25$ West of Greenwich,

which is sufficiently accurate for my present purpose, but open to amendment.

"Mr. G. H. Simmonds, who was with me for some months at Durham, is my assistant at Red Hill.

"The post-town for Red Hill letters is Reigate, Surrey."

On the Zodiacal Light. By E. J. Lowe, Esq. F.R.A.S. F.G.S.

During the past spring the observations on the *western light* have been but few, owing to the great prevalence of cloud during the early portion of the evening. However, as on several occasions some of the features were well marked, I have forwarded these records as a continuation of my former reports on this subject.

Epoch 1853, January 31st, $6^h\,45^m$.

The light was very brilliant for half an hour, the north edge being most clearly defined. The north edge passed between No. 77 and No. 82 *Pegasi*, and the south edge near *i Ceti*. The apex of the cone advanced and receded in half-minute intervals as much as 5°, its greatest altitude being *ε Piscium:* owing to a stratus stretching along the valley its extent on the horizon could not be well observed; by producing the two edges it was thought to cut the horizon 10° N. of W. and 30° S. of W.

Epoch 1853, February 27th.

Faint and confused; aurora borealis occurring at the time.

Epoch 1853, March 8th, $7^h\,45^m$.

Somewhat brilliant, yet only occasionally well defined on the edges. It had pulsations of greater and less brilliancy, and at times the north edge was clearly defined; at such periods the north edge passed about 10′ S. of *γ Arietis;* the light seldom reached this star, being mostly midway between *γ* and No. 2 *Arietis:* the star *η Ceti* was also near upon the edge. The cone rose to the altitude of *Saturn*, but was slightly north of that planet, the apex being midway between *Saturn* and the *Pleiades in direction*, but not higher than the altitude of *Saturn*. The base was hidden by a stratus. The stars were alternately dimmed and again bright beneath it; some were constantly disappearing, perhaps twice a minute, more especially in the centre of the phenomenon: once bars, or streams of light, were apparent on the north edge. At this moment the light was brightest, and the north edge most clearly defined.

Epoch 1853, March 29th, $8^h\,30^m$.

The light tolerably bright. The south edge passed midway between *o Tauri* and *λ Arietis;* the north edge *did not keep a fixed position*, fluctuating from 2° beyond *α Arietis* to midway between *α* and *β Arietis;* the apex also oscillated from the *Pleiades* to 2° beyond that group (being an oscillation of 2° vertically). The extent on the horizon was 30°. The axis passed through *π Arietis*. The light was brightest along the axis. Stars were slightly dimmed; they also did not scintillate so much *within* as *without* the phenomenon. At one period ($8^h\,45^m$) there was a double edge on the north side, the inner (or new edge) being very brilliant, and passed 4° W. of *α Arietis*. This second edge was visible 5^m. The pulsa-

tions of brightness were in about half-minute intervals, both horizontally and vertically at the same time. The phenomenon was most brilliant when most expanded.

I feel persuaded that the periodical change in brightness and the expansion and contraction of the cone is not produced by our atmospheric influence, as it is always apparent when the phenomenon is brilliant, whatever be the hygrometrical state of the air. It appears to me to be owing to *a rotation of the cone*, and that the light is not always *equally bright* in all parts. In those countries where the zodiacal light is very brilliant it would be well to observe these features carefully.

On the Telescopic Appearance of Saturn. By the Rev. W. R. Dawes.

"The present autumn has hitherto afforded so few opportunities for delicate observations with high powers, that the following results of two or three examinations of *Saturn*, under favourable circumstances, may not, it is hoped, be unacceptable to the Society.

"1853, Oct. 24, $12^h\ 45^m$ G.M.T. $8\frac{1}{2}$-foot refractor, powers 350, 460, and 640. When best seen, the *dark line* on the outer bright ring (A) was so plain that it could scarcely be overlooked during careful scrutiny of the ring. Occasionally, when particularly fine views occurred, the line seemed to extend itself a long way round; but usually not further than a tangent to the inner edge of the inner bright ring (B). It seemed equally visible at both ends of the ring, and occupies precisely the same position as it did last year and the year before, viz. just outside the middle of the ring.

"The nebulous ring (C) was extremely obvious, and struck me as rather *brown*. At times there seemed to be an exceedingly narrow dark line between the rings B and C at the ansæ, but I could not feel sure of it. The interior edge of the ring C was very well defined, and the diminution of its brightness towards its inner edge seemed to me unquestionable. The ball was very plainly seen through the nebulous ring; more plainly than I ever saw it before; the circumstances being more favourable than any of which I was able to avail myself last year.

"The *belts* are not so decided as they were last year, except the narrow belt round the south pole, which is very complete and uniform. It is of a neutral tint; but the southern hemisphere of the planet, south of the broad brownish band near the equator, certainly has a greenish hue. The broad band is not distinctly separated from the lighter shade south of it; nor is it decidedly divided into two by a bright line as it was last year, though its surface is obviously not uniform.

"While scrutinising the southern portion of the planet, I was struck with the appearance of a small black spot on the *eastern* side of the ball upon the ring B, just within the division of the rings A and B. When most sharply defined, it appeared somewhat triangular. It is, no doubt, the same as was noticed by Mr. Lassell

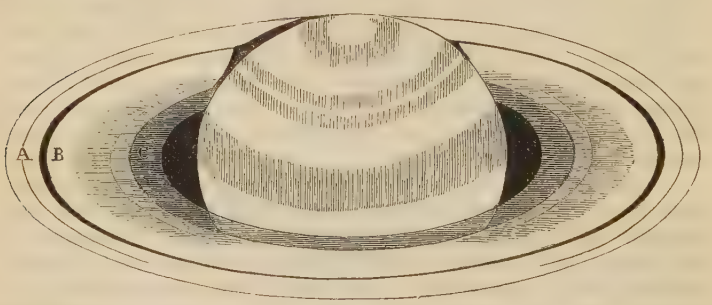
last year at Malta, and seems to be the projecting part of the shadow of the ball on the ring B. But I was amazed to see it so early, while the shadow and the *western* side is yet so broad. The edge of this shadow is most decidedly *convex* towards the ball on the ring B, and *concave* towards the ball on the ring A. There is an obvious *notch* on the edge of the shadow, which extends much further from the ball at the outer edge of B than at the inner edge of A, imparting the impression that these two edges are not in the same plane. It is far greater than is due merely to the *separation* of the two rings.

"Oct. 25, 11^h G.M.T. I entered in my journal as follows:—'Fine views, confirming all I saw last night. Vision is more steady and satisfactory. The black spot on the ring B is seen, I think, more plainly than last night. The southern edge of the ball seems to be exactly coincident with the southern (exterior) edge of the outer ring. The dark line on that ring at times very plain and steady. Nebulous ring very well seen; *western end browner than the other*. Quite transparent where it crosses the ball.'

"'Nov. 2. There is a thick haze over the sky, through which the brighter stars are shining dimly with constantly varying brightness. *Saturn* at times remarkably distinct. The black spot on ring B to the *east* of the ball is well seen: it has certainly increased since Oct. 24 and 25, when I first saw it.

"'The nebulous ring is wonderfully well seen. When the haze thickens, the fading (or shading off) of the light of this ring on the portion towards its inner edge is very obvious.'

"I inclose a small sketch of the planet, which, though it has no pretensions to great accuracy in the proportions, and shows the shadows somewhat exaggerated, may serve to illustrate the verbal description.



"*Wateringbury, 9th Nov.*, 1853."

On the Visibility of the Dark Ring of Saturn in a moderate Telescope. By T. W. Burr, Esq.

"As everything which brings a phenomenon within the reach of a larger number of observers is useful, you may, perhaps,

think it worth while to mention in the *Monthly Notices* that I find the dusky ring of *Saturn*, which I believe has only been seen by rather large telescopes, is discernible with much smaller instruments; and that the possessors of such may look for a sight of this interesting appendage to the planet with every chance of success. On the night of Thursday, 20th October, the slate-coloured interior ring was beautifully seen in my telescope of only 4 feet focal length, with an aperture of $3\frac{3}{8}$ inches. The eye-piece was an Huyghenian one, giving a power of 480. Not anticipating that the telescope would show it, I had not looked for it, but had my attention drawn to it by the inquiry of a friend, 'Why half the space between the ring and the planet was lighter than the rest?' Upon looking myself, I immediately recognised the dusky purplish light and well-defined inner boundary of the ring in question, occupying about a third of the space from the bright inner ring, and also saw its dark line across the body slightly broader at the ends than in the middle. Other persons also verified the observation. This was between 10 and 11·30 G.M.T. The atmosphere was excessively moist, and the sky rather hazy; but I have never seen the detail of the moon and *Saturn* to greater perfection than on that evening. I do not suppose that many telescopes so small as my equatoreal will show this ring, as my object-glass (by Ross) is a very fine one; but I should think good 5-feet telescopes, of $3\frac{1}{4}$ inches aperture, which are in the possession of so many amateurs, would."

On Rating Chronometers by Lunars. By H. Toynbee, Esq., Commander of the Gloriana, East Indiaman.

In vol. ix., No. 7, of the proceedings of the Royal Astronomical Society, dated May 11, 1849, will be found the notice of a paper "On the Practicability and Advantages of obtaining a Sea-rate for a Chronometer," which is published entire in the *Nautical Magazine* for February 1850.

In the paper alluded to, I endeavoured to prove how a good practical observer of lunars could obtain a better sea-rate for his chronometer than that supplied by the maker, and than one determined by other means during the ship's stay in port, also an original error which very rarely gives an error on Greenwich mean time of more than twenty seconds.

A general opinion seemed to be that I was correct, so far as my own observations were concerned, but that lunar observers generally could not apply the method satisfactorily.

My present object is to prove this objection erroneous. Late in 1850 I took the command of another ship, my chief officer during this voyage, on account of the delicate state of his health, was prevented from taking lunars.

In 1851, Mr. J. W. Quihampton (now commander of the Tudor) became my chief officer, and continued so until the middle of September 1853. He had been in the habit of taking lunars, but, like almost all persons with whom I have conversed on the subject, had been taught that the best lunars could not be depended upon

within fifteen miles of longitude, so did not think it possible to rate chronometers by them.

This opinion (which I believe to be almost universal amongst seamen) must be set aside, and the method, which is fully explained in the *Nautical Magazine* for February 1850, fairly tried. Mr. Quihampton did so, and I proceed to give a comparative view of the errors and rates of Dent's 1759 chronometer, found by his observations and my own, together with those by the gun at Madras, the ball at St. Helena, and the maker on arriving in England.

	Mr. Quihampton's Original Error. Fast of G.M.T.	Daily Rate. Gaining.	My Own. Original Error. Fast of G.M.T.	Daily Rate. Gaining.	Various Sources. Original Error. Fast of G.M.T.	Daily Rate. Gaining.
	m s	s	m s	s	m s	s
May 14	⊙ E. ☾ 38 19·3		⊙ E. ☾ 39 22·3			
	⊙ W. ☾ 40 10·7	8·9	⊙ W. ☾ 39 31·1	9·4	Flash of Madras Gun.	
	Mean 39 15		Mean 39 26·7		39 22·7	9·3
30	⊙ W. ☾ 42 43·4		⊙ W. ☾ 42 19·1			
	⊙ E. ☾ 41 18·9	9·5	⊙ E. ☾ 42 55·4	10·5		
	Mean 42 1·1		Mean 42 37·3			
June 12	⊙ E. ☾ 43 17·3		⊙ E. ☾ 45 4·0			
	⊙ W. ☾ 44 34·4	9·1	⊙ W. ☾ 44 20·1	9·9		
	Mean 43 55·8		Mean 44 42·1			
July 2	⊙ W. ☾ 47 34·4		⊙ W. ☾ 46 49·5			
	⊙ E. ☾ 46 16	9·0	⊙ E. ☾ 47 6·3	7·6		
	Mean 46 55		Mean 46 57·9			
13	⊙ E. ☾ 47 45·1		⊙ E. ☾ 48 23·3			
	⊙ W. ☾ 48 45·8	8·1	⊙ W. ☾ 47 54·7	7 0	St. Helena Ball.	
	Mean 48 15·4		Mean 48 9		47 54·5	
30	⊙ W. ☾ 51 6·9		⊙ W. ☾ 50 7·2			
	⊙ E. ☾ 50 9·5	8·3	⊙ E. ☾ 50 47·8	7·9		
	Mean 50 38·2		Mean 50 27·5			
Aug. 10	⊙ E. ☾ 51 50·7		⊙ E. ☾ 52 21·3			
	⊙ W. ☾ 53 2·9	9·2	⊙ W. ☾ 51 52·3	8·5	Maker's Error brought back by Maker's Rate.	
	Mean 52 26·8		Mean 52 6·8		52 9·5	
15	.....		.....		Maker's Error and Rate. 56 47·5	8·0

Note.—Each date applies to all the results on the same line with it.

We were only one day in St. Helena, so could obtain no rate by the ball. The ship arrived in the East India Docks on the 13th September, the chronometers were taken by the maker on the 14th, and the daily rate gradually decreased to 6ˢ·5 during the first five days.

On referring to the above table it will be noticed that in Mr. Quihampton's case, the ☉ W. ☾ lunars invariably give the greater, but in mine almost as invariably the lesser error. This may arise from a constant error in the observer, or his instrument; but the means of our ☉ E. ☾ and ☉ W. ☾ errors nearly agree.

The differences between our rates found at the same time, the mean of which is eight-tenths of a second, are reduced by taking a mean between the new rate and the one previously found. In cases where the chronometers, by their second differences, prove themselves to have been going regularly since the previous rate was obtained, I strongly recommend this to be done.

Since leaving Madras, Mr. Quihampton has rejected three sets of sights, and I two, they were deemed but indifferent observations before working them. In several instances lunars were obtained on only one day of the sun and moon's continuing in distance.

Both sextants were by Cary. Mr. Quihampton always used green and red shades, I a blue one; the same shades were always used, and the instruments placed as near as possible in the same state for each observation, as more fully explained in my first paper.

We each took and worked two sets when practicable, to check errors of calculation. They were both taken at the same sitting. Mr. Quihampton's set consisted of three, mine of five, sights.

The first part of the voyage was to Port Phillip, where the chronometers were not rated, still we found on arriving at Madras that by applying the lunar errors we had the Greenwich time within a few seconds.

During the passage from Madras to London, we had a furious gale off the Cape on June 23, 1853, and the second difference of the chronometers decreased twelve seconds before arriving at St. Helena, still we hove to at 4 A.M., July 12th, and at daylight found the island bearing north-west ten miles as expected.

Early on the morning of Sept. 8th we sighted the Eddystone lighthouse, when the cross-bearings and sights computing with the lunar error, placed us in the same position to a mile and a half.

I think the preceding table shows why lunars are not considered correct within fifteen miles of longitude, because most of Mr. Quihampton's errors, taken separately, give the longitude that much wrong. It was the same with mine until the sextant was carefully adjusted by the maker.

I think it has also proved that there is a constant character in lunars taken by the same observer, using the same instrument in the same state; whereby any person having practised this method for a short time, may readily show how much a single set of lunars gives the Greenwich time in error. For instance, Mr. Quihampton's ☉ W. ☾ and ☉ E. ☾ lunars differ in the Greenwich time about one minute and twenty seconds; now the half of this, or forty seconds of time, may be called his constant error; mine differ about twenty-nine seconds, so that my constant error is fifteen seconds of time, which applied to any single set of observations will give the

Greenwich time very nearly correct: hence an observer having ascertained his constant error, has the true longitude from a single set, taken either ☉ E. ☾ or ☉ W. ☾.

This voyage my constant error has been very nearly the same throughout. Last voyage the ☉ E. ☾ lunars gave eighteen seconds more subtractive error than the ☉ W. ☾ on the passage out to India, and thirty seconds less during the passage home. I am of opinion that this difference arose from a change in the state of the sextant during our stay in port.

What I consider to be new in this method is, its enabling an observer of lunars to find rates for chronometers each fortnight of his voyage, and so to mean ☉ E. ☾ and ☉ W. ☾ lunars—though several days have elapsed between the observations—that he obtains an original error for chronometers, which is very near the truth, and a constant error for himself, by the application of which a single set of lunars gives the true longitude. I have practised the method for nearly five years, and it has never misled me in determining the ship's position. My knowledge of the general accuracy of other observers convinces me that they may do the same, which, I think, this paper proves.

It cannot be too generally known that lunars a month old are very valuable. I have the calculations from which my errors and rates are deduced. They are at the service of any person wishing to see them.

Report of the Radcliffe Observer to the Board of Trustees, July 1, 1853.

The principal event reported upon is the completion of the electric communication between the Radcliffe Observatory and the Royal Observatory of Greenwich. The difference of time between the two Observatories has not yet been determined.

Since the last meeting two volumes of the Radcliffe Observations have been published, which bring down the published records of the Observatory to the end of 1851. The Observations for 1852 are stated to be ready for the press.

The calculations for the Catalogue of Circumpolar Stars, which were committed to Mr. Luff, have been brought down to the period of the last published volume; and it is expected they will be altogether completed before the next meeting of the Board.

Mr. Johnson proposes printing the Catalogue separately as a supplement to the fourteen volumes of Observations, of which it will contain the substance, retaining the same form and type.

The Heliometer continues to maintain its high character. Two years' experience has pointed out certain minor faults, which Mr. Johnson believes to be remediable; but he is waiting the completion of a long series of observations on which he has been engaged before he communicates with the maker on the subject. Such is the power of the instrument, that he believes a change of

relative position of two stars, marked by the $\frac{1}{4000}$th part of an inch on its scale, would not be undetected.

Steps have been taken for the construction of a Photographic Barometer and Thermometer. Mr. Johnson hopes, at the next meeting, to be enabled to report a considerable improvement in the meteorological department of the Observatory.

The Report concludes with some suggestions relative to the internal arrangements of the Establishment.

Remarks on the Erection of the Time-Ball of the Royal Observatory, Edinburgh. By Professor Piazzi Smyth.

The simplest principle of a time-signal is to let fall some sort of conspicuous body at the appointed instant. To be seen at a distance, it must be large; to be equally visible from all directions, it must be spherical; and to be well seen, projected as it will always be here on the sky behind the lofty Nelson's Monument to all observers except such as may be on higher ground still, it requires to be painted black. Again, that it may always descend with equal velocity, whether the day be calm, or whether there be much wind pressing the ball sideways against its supports and retarding its descent by friction, it must be heavy; and, in order that the observation be of as exact a nature as possible — in order that the instant of the *beginning* of the descent may be observed with the utmost accuracy — there must be a cross staff on the top of the mast, up to and in contact with which the ball is to be hauled, and the instant of separation of the ball in its descent from the said cross staff indicates the moment of the *beginning* of the fall with abundant precision.

We have thus a large and heavy ball (say four feet in diameter) hauled up to the top of a mast with cross-bars on the summit, and made to fall at a certain instant daily. That is the simple principle; but what takes place in practice? Firstly, such a ball falling a great height would soon break itself to pieces; and the stronger it is made, the heavier it must be, and the more suicidal force it will have. Secondly, the effect would be also that of a ramming engine to destroy the structure whereon the ball fell, in the present case the many-storied tower of Nelson's Monument, with all its primitive architectural decorations.

The earliest signal-balls which were made, though provided with ropes passing over pulleys by which they were enabled in their descent to raise a series of weights in order to check in a gradual manner the velocity of their fall, were yet invariably found, after a short time, to pull or to smash themselves to pieces. Steel springs were next tried to break the force of the concussion, but were pretty sure to be themselves snapped with a heavy ball, while a light one would not descend quick enough on a windy day. Recourse was finally had to compressed air, a spring of perfect temper

never injured by time, and capable of any degree of delicacy at first, and any amount of violent resistance at last.

To carry out this principle a staff was attached to the ball below, terminating in a piston, which in the course of its descent entered an accurately turned cylinder, and compressing the air therein, was gradually brought to rest. Were the cylinder quite closed at the bottom, the spring of the included air might be greater than required, and also have a tendency to throw the ball up the mast again, which would be somewhat troublesome to observers. But by simply opening a graduated aperture below, so as to admit of the air partially escaping as it is compressed, the strength of the spring is diminished, and by the time that the piston has descended to the lowest point, there is so little air remaining in the cylinder, and it is still escaping so fast, that there is no power left to make the ball rebound.

Thus the time-ball is made to descend without injuring the building or spoiling itself; and the trigger apparatus, by which the detent, that holds the ball when hauled to the top of the mast, is unlocked, being very nicely adjusted, and observers being duly cautioned to look to the instant of *separation of the ball from the cross-staff*, the descent — *i. e.*, this first part of it — is as instantaneous as need be. In the next place, the trigger being pulled, not by the finger of a person at the ball, but by an electro-magnet which is instantaneously set in action by the contact made at the end of a wire led into the walls of the Observatory, and brought immediately before the transit-clock itself; the instant for the signal outside can be conveyed to the undeviating mechanism there with all the refinement of a chamber experiment, and to the utmost extent of the observer's knowledge of the real time by the stars, as obtained the previous night, and continued on by the clock.

When the weather permits the stars to be observed, the time may be depended on to less than one-tenth of a second. But when, as too frequently occurs in this country, with its cloud-diversified sky, more attractive to the painter than suitable to the astronomer, no star may have been seen for several days, the clock may deviate considerably; nay, so much, that on such days it would be better not to drop the ball at all, rather than to give an erroneous indication.

But here, again, Science affords her assistance. Such unmitigated bad weather seldom prevails equally in the south of England and over this city at the same moments; consequently an observation is often possible, and can be well obtained at Greenwich, when quite out of the question here. Then by refinement of galvanic agency the instants of such an observation, the moments of the star being on the fiducial lines of the Greenwich transit, may be flashed along a wire and indicated on any other clock, as that of our own Observatory; if only there be complete wire connexion the whole of the way. Such a road for the electricity there has long been from the Greenwich Observatory to the railway station in Princes Street; but there being none thence to the Calton Hill, it could not be used for exact purposes. Within the last few days, however, the

Government has consented to the wire being brought up to the Observatory, on the strong representation of the importance of the measure by the Board of Visitors of the Royal Observatory, under the presidency of the Right Hon. Lord Rutherfurd, who, we may add, had long since evinced the warmest interest in the subject of the time-ball, and, as Lord Advocate, three years ago, was the important turning point of the whole scheme.

Nearly every contingency seems thus to have been prepared for, except, perhaps, the conveniency of the method of hauling up the ball, as influenced by the peculiarities of the site—viz. the apparatus being on the top of a lofty building, and at a distance from the Observatory.

With reference to this object, it may be remarked, that, some trouble might be experienced in ensuring a porter's daily attendance throughout the year with perfect punctuality, unless recourse is again had to modern science. In accordance with this, a plan has been proposed, by which a weight having been wound up at *any previous hour of the day or night*, then on electrical contact being made at the Observatory by the astronomer at a precise moment, that weight is unlocked, immediately descends, and hauls up the the ball. Next at five, or any other number of minutes, a second contact being made on another wire lets the ball down.

Thus the ball is both raised up and let down by delicate signals given from the interior of the Observatory by the astronomer himself with equal punctuality, both operations being similarly independent of any external person at the time. Hence arises a result not unfrequent when human labour previously employed in the mere brute purposes of raising weights or turning wheels is replaced by scientific machinery, that the work is not only cheaper but better done than before. For the ball may in this way be raised so quickly and certainly, that the beginning of its rise becomes as good a time-signal as the commencement of its fall. There may thus be two signals daily; two also which can never be mistaken for each other, as two successive descents might often be.

Report of the Syndicate appointed to Visit the Observatory of Cambridge.

In this Report an account is given of the labours of Professor Challis at the Observatory of Cambridge during the year 1852, and also from the commencement of the present year down to the 20th of May.

The first part of the Report relates to the observations made in the year 1852. The transit observations made during the course of the whole year amounted to 2050, and the circle observations to 1543. Of these, 362 transits and 340 circle observations relate to bodies of the solar system. The remaining observations of stars were made for the most part in pursuance of Prof. Challis's plan of forming gradually a catalogue of zodiacal stars situated within 5° of the ecliptic, to the ninth magnitude inclusive.

Observations of seventeen of the newly-discovered planets and of three comets (Biela's, Encke's, and Westphal's) were made in the course of the year with the Northumberland equatoreal. The comparisons in right ascension amount to 682, and those in north polar distance to 678. Eight occultations of fixed stars by the moon were observed.

At the beginning of the present year Professor Challis was induced to modify the system of meridian observation hitherto pursued by him, having found it to be no longer practicable to observe on the meridian with sufficient attention the large and constantly increasing number of newly-discovered small planets. The observations with the meridian instruments are now confined to those of the sun and moon, while the newly-discovered planets and comets are observed exclusively with the Northumberland equatoreal; the former more especially, in those positions wherein it is impossible to observe them on the meridian with the transit circle of the Greenwich Observatory.

The transit observations taken from the beginning of the current year to May 20 were 795, and the circle observations 553. In the same interval the Northumberland equatoreal was employed in observations of *Calliope*, *Lutetia*, *Thalia*, *Hygeia*, *Parthenope*, Secchi's Comet, *Iris*, and *Victoria*, the comparisons of these bodies with stars in right ascension amounting to 298, and those in north polar distance to 296. Twelve occultations of stars by the moon have also been observed.

Professor Challis has applied Mr. Pritchard's invention for getting rid of the oscillations at the surface of mercury, which occasion so much trouble to astronomers in observations by reflexion. The tremor is found to be much less than in a wooden trough. Further trial, however, is necessary before the new trough can be pronounced to answer the intended purpose in all respects.

The Report concludes with an allusion to the operations for determining the longitude of the Observatory of Cambridge by means of galvanic signals.

Tables of the Moon, &c. arranged in a form designed by Professor Benjamin Peirce, under the superintendence of Charles Henry Davis, Lieut. U.S.N., and published under the authority of the Hon. John P. Kennedy, Secretary of the Navy. Washington, 1853.

A review of the Lunar Tables constructed in successive ages presents an interesting picture of the progress of astronomers in unfolding the complicated irregularities of the moon's motion. It is a remarkable fact that the most considerable inequalities in the longitude of this body due to perturbation were satisfactorily established, not only previous to the discovery of the theory of gravitation, but even before any conception had been formed of the elliptical movements of the various bodies of the planetary system. It was by the aid of the much-decried theory of epicycles that astronomers were enabled to arrive at these important results.

Thus the discovery of the evection by Ptolemy was the direct consequence of a comparison which that astronomer instituted between the observations of the moon in quadratures and the corresponding places assigned by the epicyclical theory. Again, in modern times, when Tycho Brahé succeeded in bringing practical astronomy to a state of excellence hitherto unknown, the same theory was instrumental in conducting that astronomer to the discovery of the variation and the annual equation.

The elliptical theory which forms the basis of the famous Rudolphine tables, published by Kepler in 1627, was not applied very successfully by that astronomer to the moon. In representing the equation of the centre, he, indeed, experienced no difficulty; but his explanation of the evection was very unsatisfactory, being, in fact, a remnant of the epicyclical theory engrafted upon the elliptic orbit of the moon. Horrocks first showed that the evection might be completely reconciled with the elliptical theory, by supposing the eccentricity of the lunar orbit to be variable, and ascribing a libratory motion to the line of apsides. The Lunar Tables by Flamsteed, inserted in Moore's *Mathematics*, published in 1681, may be considered as the most complete embodiment of all the facts relating to the moon's motion that had been established by astronomers previous to the publication of the *Principia*. It may be remarked, that to Flamsteed is also due the merit of having first given the right explanation of the annual equation, the origin of which had hitherto been imperfectly understood by astronomers.

Newton, by his discovery of Gravitation, was enabled to establish the existence of several inequalities in the moon's motion, which had hitherto escaped the notice of astronomers. It is well known that his *Theory of the Moon* was not published in the original edition of the *Principia*. It first appeared in the year 1702, in the *Astronomiæ Elementa* of David Gregory, and was subsequently inserted in the second and third editions of the *Principia*. Four new inequalities in the moon's longitude were assigned by him as deducible from the theory of Gravitation. This made the whole number of the inequalities in the longitude now to amount to eight.

Newton's lunar theory continued for several years to form the basis of a succession of lunar tables, which were constructed by various individuals both in England and on the Continent. In 1749, Halley's Astronomical Tables, which had been printed off as early as 1719, were finally published. The lunar tables were similar to those which had been already calculated from Newton's theory, except that one of the four new equations was altogether omitted.*

The purely analytical researches of Euler, Clairaut, and D'Alembert, on the lunar perturbations, had the effect of introducing into the theory of the moon's motion a multitude of minute inequalities in addition to those discovered by Newton. Tables based on their respective theories were constructed by these geometers. Euler's

* Viz., the equation $2' 20''$ sin $(\mathbb{C} - \odot)$. The value of the coefficient of this inequality, according to Plana, is $2' 2''{\cdot}1$.

Tables were first published in 1746 (*Opuscula Varii Argumenti*, p. 137), and the theory upon which they are founded in 1753. Subsequently there appeared the results of his more mature researches on this subject, contained in a ponderous quarto volume (*Theoria Motuum Lunæ, Nova Methodo pertractata*, Petrop. 1772). New tables were given by him on this occasion, constructed upon a plan altogether novel, and which was suggested by the peculiar form of his theory. The moon's place was expressed by means of rectangular co-ordinates referred to the ecliptic and a plane perpendicular to it. When these were once calculated from the tables, it was easy to deduce from them the longitude, latitude, and radius vector. Clairaut's Lunar Tables were first published in 1752 (*Théorie de la Lune*, St. Petersbourg.) A revised edition subsequently appeared at Paris in 1765.* D'Alembert's tables were published first in 1754 (*Récherchcs sur différents Points du Système du Monde*), and afterwards in 1762 (*Opuscules Mathématiques*, tom. ii.).

Notwithstanding the high position occupied in the mathematical world by the three geometers just mentioned, the lunar tables constructed by them did not possess such a degree of accuracy as might have been expected from the immense developement which the expressions for the moon's co-ordinates had acquired from their researches. This may be attributed partly to the imperfect state of the lunar theory, which, as a subject of analytical investigation, could only be considered as yet in its infancy, and partly to a want of practical skill on the part of the geometers themselves, in correcting the results of theory by comparing them with an extensive series of actual observations of the moon's place.

The Lunar Tables of the immortal Mayer were first published in 1753 (*Acta Reg. Soc. Gott.*, vol. ii.). A corrected copy was shortly afterwards sent to London for the consideration of the Board of Longitude, and at his death, in 1762, his widow transmitted to this country a third and still further revised copy. It is well known that the Board of Longitude adjudicated favourably on the merits of these tables, and that the widow of Mayer received on account of them a considerable sum of money from the British Government. They were published at London in 1770.

The number of arguments used in calculating the moon's longitude by Mayer's Tables was fourteen. This was one-third less than the number used by Clairaut. Still it was universally admitted that the Tables of Mayer represented the moon's place with greater accuracy than any which had hitherto been constructed. The physical theory upon which these Tables were founded was in all respects identical with that which formed the basis of the Tables

* In this edition of Clairaut's tables the number of arguments for the moon's longitude, including the argument for the reduction, is twenty-one. The following tribute paid by the author to the illustrious Lacaille is worth quoting:— "C'est feu M. l'Abbé de la Caille qui m'avait penser à employer une colonne pour le supplément du nœud, au lieu d'employer celle du nœud même, ainsi que la distance de la lune au pole de l'écliptique à la place de la latitude." (*Théorie de la Lune*, p. 115.)

constructed by Euler, Clairaut, and D'Alembert; but their author was enabled to diminish considerably the number of arguments by means of a transformation suggested naturally enough by his analytical researches. In the expression for the moon's true longitude, as *directly* deduced from theory, the arguments of the various inequalities were formed by a series of combinations of the epochs and *mean* motions of the sun and moon. It occurred to Mayer, however, that as the *mean* longitude of the sun enters into the expressions for the perturbations of the co-ordinates of the moon's place only in so far as it depends upon the actual position of the sun, which is the disturbing body, the number of inequalities in each case might be considerably diminished by employing equivalent expressions depending on the *true* longitude of the sun. In the Tables of that astronomer the true longitude of the sun is accordingly used, instead of the mean longitude, as in those of Clairaut. Another peculiarity of these Tables, the necessity for which was occasioned by the substitution of the true for the mean longitude of the sun, consisted in the circumstance that after the first ten equations were calculated, the moon's mean longitude in the remaining arguments was successively corrected by adding to it the sum of the preceding equations.

The Lunar Tables of Mayer, on account of their accuracy and the comparatively small number of arguments which they contained, were generally employed for calculating the moon's place in preference to any others which had been hitherto constructed. The use of them, however, was attended with two obvious disadvantages. In the first place, it was indispensable as a preliminary operation to determine the sun's true longitude by means of the solar tables. Again, since the arguments did not vary proportionably to the time, it was impossible to accomplish their *direct* formation by means of tables prepared beforehand.

In 1780, Mason's Tables of the Moon were published. In their construction and arrangement they resembled Mayer's tables; but the coefficients were slightly altered, and eight small equations were added to the longitude which Mayer had expressly omitted on the ground of their being insensible. The number of arguments employed in calculating the moon's longitude amounted, therefore, to twenty-two. These Tables were afterwards published by Lalande in the second edition of his Treatise on Astronomy (1792).

In 1806, Bürg's Lunar Tables were published under the auspices of the French Bureau des Longitudes. Their mode of arrangement did not exhibit any novelty. The eight new equations added to the longitude by Mason were found by Bürg to be indispensable. To these he added other six very small equations. The entire number of arguments employed in the calculation of the moon's longitude now amounted, therefore, to twenty-eight. Of these, the first twenty-four were calculated independently, and then, as in Mayer's Tables, the mean longitude of the moon in the remaining arguments was successively corrected by adding to it the sum of the preceding equations.

Soon after the publication of Mayer's Lunar Tables, the inconvenience attending the formation of the arguments had suggested the desirableness of transforming the expressions for the moon's co-ordinates, assigned by that astronomer, into analogous expressions depending upon *mean* arguments, with a view to the construction of new tables. In the *Memoirs* of the Berlin Academy for 1781, Schulze has, by a most laborious process, effected the transformations necessary for this purpose. It does not appear, however, that any further progress was made in the construction of tables adapted to this form of the arguments.

Another important disadvantage connected with Mayer's Tables, and all others constructed upon the same plan, consisted in this, that they did not admit of being compared directly with theory. In order to the attainment of this end, it was indispensably necessary either that the arguments of the tables should undergo a transformation similar to that which Schulze had effected for Mayer's tables, or that the expressions for the moon's co-ordinates assigned by theory should be reduced to the form employed in the construction of the tables. In the *Mécanique Céleste* (tome iii. p. 275, *et seq.*), Laplace has transformed the arguments employed in the Tables of Mason and Bürg into mean arguments, for the purpose of comparing the results with his own theory, and in the *Monatliche Correspondenz*, 1804, p. 232, Bürg has effected the converse of this process. In the *Connaissance des Temps*, for the year XV. of the Republic, Burckhardt has transformed Laplace's formula for the moon's parallax into an expression depending on the true longitude of the sun, and compared the results with Mayer's Tables. Mr. Adams, however, has recently shown that no reliance can be placed on the conclusion which Burckhardt deduced from this comparison.

The publication of Carlini's Solar Tables, which originally appeared in the *Milan Ephemeris* for the year 1810, marks an important epoch in the construction of Astronomical Tables in general. In these tables the arguments consist of a series of arcs, varying uniformly with the time, and their values are expressed not in space but in *time*, the cycle of their values being represented, in each case, by the *period* of the inequality. Hence, if it was required to calculate the place of the sun for any assigned date, the tables furnished the values of the arguments for the nearest epoch, and then their values for the actual date were obtained *by adding to each of the tabular values the common interval which elapsed between the epoch and the date.* The simplicity of such a process is obvious at first sight, more especially if there should be a great multitude of arguments of different degrees of magnitude. Carlini suggested the advantages of this form in the construction of lunar tables, and he pointed out some of the most important modifications which would be necessary in that case in order to ensure its practical utility.

In 1812, Burckhardt's Lunar Tables were published. Although constructed according to an artificial form, like those of Mayer, Mason, and Bürg, they differed materially, in several respects from

any of the latter. The *mean* longitude of the sun was used instead of the *true*, and the arguments were expressed in terms of the centesimal division of the circle. The number of arguments in the moon's longitude was thirty-six. Of these, thirty-two were formed independently, and the mean longitude of the moon in the remaining arguments was then successively corrected by adding to it the sum of the preceding equations. The calculations founded on these Tables are seriously vitiated by an important error in the expression for the parallax, which Mr. Adams has recently detected.

In 1824, appeared Damoiseau's Tables of the Moon, founded solely on his own theoretical researches. A new edition of these Tables was published in 1828, in which the centesimal division of the circle, which was originally used in expressing the values of the arguments and equations, was replaced by the sexagesimal division. Mr. Airy has justly censured this retrograde step.* The various inequalities are exhibited in the form in which they are deduced from theory. The number of arguments in the moon's longitude is forty-seven. Of these, thirty-two are formed independently. The remaining fifteen are compounded of these, and the values of the corresponding equations are found by means of tables of double entry.

The Tables of Damoiseau exhibit a return to the form of construction employed in the Tables of Clairaut. The process for calculating the moon's longitude is exactly the same in both cases, with the exception of being much longer as regards Damoiseau's Tables, in consequence of the greater number of arguments.

The Tables employed by Mr. Airy in the Reduction of the Greenwich Lunar Observations are a modification of Damoiseau's, suggested by the theory of Plana. The calculations of the moon's place that have been required for this great operation are the most extensive that have been hitherto executed by the aid of the lunar theory in its most developed state.

The American Lunar Tables are constructed from Plana's theory, modified by the results of the more recent researches of Airy, Hansen, and Longstreth. The plan of construction is that recommended by Carlini. First of all, the date for which the moon's place is to be calculated, is expressed in mean solar days, counting from the beginning of the Julian Period. If it falls anywhere between 2300000 and 2400000 days (or nearly between 1585 A.D. and 2131 A.D.), the tables at once assign the values of the epochs and arguments for the nearest thousand of days, and the number of days which remain over the thousands is then placed under each argument. The sum of these two quantities constitutes the value of the argument for the date, except in a few instances, in which a small correction for secular variation requires to be added. As the value of any argument thus found frequently exceeds many complete periods of the inequality to which it refers, tables are given containing a series of multiples of the period, so that the nearest multiple being subtracted from the gross value of the argument as already found, the remainder, being less than a whole period, is immediately

* Reduction of the Greenwich Lunar Observations, Introduction, p. lxxi.

available for ascertaining the value of the inequality, by means of tables adapted to the successive values of the argument.

Tables are given for computing the values of the arguments in those cases wherein the date does not fall within the limits above mentioned. In such cases, as might naturally be expected, the process is not so simple.

In the tables which assign the relation between the arguments and the equations, the values of the latter are generally given for every hundredth of a day of the argument. It is but right to state that Carlini distinctly pointed out the necessity for this subdivision of the day in the application of arguments of time to the construction of lunar tables, and also recommended the use of tables of multiples of the periods for the purpose above mentioned. (*Effem. Mil.* 1810.)

The simplicity of the process for forming the arguments cannot fail to constitute a strong ground of recommendation in favour of these Tables. For the calculation of an ephemeris of the moon's place, they seem to be especially adapted. At the same time it must be admitted that the immense number of arguments, amounting to seventy-nine for the longitude, presents a serious obstacle to their practical utility. It may be remarked, also, that the recent researches of Mr. Adams on the lunar theory have tended seriously to invalidate more than one of the elements employed in their construction.

The arrangement of these Tables is stated to have been the result of a plan devised by Professor Peirce. It is very clear and masterly, and is in every respect worthy of that eminent mathematician. It may be mentioned, in conclusion, that the work, like most scientific publications which emanate from the American press, is beautifully got up, far surpassing in this respect any Astronomical Tables which have hitherto been published.

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